



Is Formal Employment Discouraged by the Provision of Free. Health Services to the Uninsured? Evidence From a Natural Experiment in Mexico

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► To cite this version:

Alejandro del Valle. Is Formal Employment Discouraged by the Provision of Free. Health Services to the Uninsured? Evidence From a Natural Experiment in Mexico. 2013. halshs-00838000

HAL Id: halshs-00838000

<https://shs.hal.science/halshs-00838000>

Preprint submitted on 24 Jun 2013

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PARIS SCHOOL OF ECONOMICS
ÉCOLE D'ÉCONOMIE DE PARIS

WORKING PAPER N° 2013 – 19

**Is Formal Employment Discouraged by the Provision of Free. Health
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JEL Codes: I15, I18, I28, I38, J01, O12, O17

Keywords: Labor Markets, Health Provision, Informality, Spillover Effects



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Is Formal Employment Discouraged by the Provision of Free Health Services to the Uninsured ?

Evidence From a Natural Experiment in Mexico

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April 21, 2013

Abstract

This article analyzes whether the large scale provision of non-contributory health services encourages workers to move away from jobs that pay contributions to social security (formal employment). Using a difference-in-differences design, that exploits the variation generated by the municipal level roll-out of an intervention of this kind in Mexico, this paper finds that contemporaneous program exposure has no impact on the ratio of formal to total employed and that lagged exposure leads only to a small (0.78 percentage points) decrease. Two proxies of spillover effects further reveal that this estimate is robust and that the upper-bound of program effect is only moderately larger (1.5 percentage points).

Keywords: Labor Markets; Health Provision; Informality; Spillover Effects.

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Introduction

Nearly one-third of the world population has no access to health services, and most of the uncovered are concentrated in developing and middle income countries (ILO, 2008). Within these countries, over-reliance on contributory social security systems, limited to affiliated workers and their families, results in low coverage rates that are strongly skewed against those at the bottom of the income distribution.

Over the last two decades a number of countries have implemented large scale health reforms, aimed at bridging the gap between the insured and the uninsured.¹ Although, each reform has its own set of specific features, at their core there is some sort of non-contributory program.² These can range from those that fully subsidize contributions to social security for targeted groups (e.g. Chile and Turkey), to programs that result in the introduction of a separate non-contributory tier that runs in parallel to contributory social security, (e.g. Mexico, China, India and Indonesia).

This type of interventions are expected to be welfare improving, directly through their effect on health outcomes and indirectly through the related gains in productivity, human capital accumulation, and poverty reduction. Nonetheless, in the labor market, the provision of quasi-free health services to the uninsured amounts to a relative reduction in the benefits derived from contributory social security, potentially leading workers to become or remain informal, Levy (2007) and Wagstaff (2007).³

This is important because informality reduces the number of tax payers while raising the cost of providing non-contributory health services. It additionally increases the fraction of the population that is unable to access benefits that remain bundled with social contributions (e.g. retirement pensions, disability benefits), and in some scenarios it may even reduce productivity by distorting firm's investments in labor training and technology adoption Levy (2007, 2008).

Conceptually the magnitude of the effect is expected to be inversely related to the degree of labor market segmentation. Intuitively this occurs because in a competitive labor market the program may encourage workers to look for informal jobs, while simultaneously discouraging individuals from seeking formal jobs. By comparison, in a segmented labor market only the latter channel is expected to operate.⁴

The lack of empirical evidence and the frequency with which concerns over the labor market consequences of this type of intervention are raised in policy dialogues, are the underlying motivation for this article which sets out to weigh-in on the cost side of the debate, by establishing in a country with a competitive labor market, whether formal employment is reduced as a result of the large scale expansion of non-contributory health services. In particular, I focus in the case of Mexico because it provides an almost ideal setting for addressing this issue.

First, in Mexico there is little evidence in support of labor market segmentation, Maloney (2003) and Maloney and Bosch (2006). More specifically, minimum wages have not been binding for a decade, unions are primarily concerned with preserving employment, wages are extraordinarily flexible during crises, and the patterns of worker transitions between formal and informal jobs do not correspond to those of a segmented market.

Second, in 2004 the Mexican government introduced Seguro Popular (SP) in order to provide quasi-free health services to those not covered by contributory social security. The program registered 42 million affiliates by 2010, making it the largest expansion of non-contributory health services in the

¹A non-exhaustive list of countries that implemented health reforms aimed at achieving universal coverage, include: Brazil, Chile, China, Colombia, India, Indonesia, Israel, Mexico, Peru, Taiwan, Thailand, Turkey.

²See OECD (2011) for a general review and Robalino et al. (2010) for an overview focused in Latin America.

³In this article formality will be equated with contributing to social security because this is the relevant dimension.

⁴See Perry et al. (2007) for an overview of the competing theoretical interpretations and their implications.

Americas.⁵ This increase in coverage is particularly meaningful because it was accompanied by a 50% increase in average non-contributory health care expenditures, Azuara and Marinescu (2011). And while this figure cannot be equated with improvements in quality, by most accounts, SP improved access while standardizing and increasing the scope of services, Lakin (2010). Further evidence of the effectiveness of SP can be found in series of papers that document its capacity to increase the use of health services while reducing out-of-pocket and catastrophic health care expenditures.⁶

Interestingly for identification, the progressive roll out of the program at the municipal⁷ level created a source of variation across space and time that can be exploited using a difference-in-differences design, where municipalities that receive the program at an earlier stage serve as the treatment group while those that receive it later serve as the control group. In order to make a strong claim of internal validity, this paper focuses on a subsample of municipalities for which it is possible to show that pre-intervention time trends between potential treatment and control groups are parallel.

It additionally deals with time varying factors related to both the timing of SP and formal employment, by laying out the alternative objectives that federal and state authorities could have pursued when deciding program roll out, narrowing down the type of confounding factors, and then taking advantage of various data sources, from public finance administrative records to electoral results at the municipal level, in order to directly control for each of these factors.

The empirical strategy is then adjusted to deal with two additional issues. The first is expanding the analysis to estimate the effect of lagged exposure to the program. This is important because it is reasonable to assume both that individuals are unlikely to be immediately aware about the availability of SP and/or that the initial valuation of the services is low given their experience with other governments programs.

The second issue is whether municipalities could have been indirectly exposed to SP. Exploring the validity of the stable unit of treatment assumption is particularly interesting in this setting as its violation may not only be a source of bias but also of heterogeneity in program impact. Specifically, this paper uses two proxies of spillover effects to help gauge the impact of indirect exposure: road distance to the nearest municipality offering SP, and the population weighted share of neighbor municipalities with direct access to SP.

As such, this paper goes beyond a recent set of papers analyzing the labor market consequences of SP.⁸ It contributes to the literature in two ways. First, it employs an empirical strategy that allows me to derive causal estimates of both contemporaneous and lagged program impact under far less stringent assumptions. Second, it accounts for the role of indirect exposure to SP, this bolsters the reliability of the estimates, allows me to derive an upper-bound of SP impact and provides insights into the mechanisms that are possibly limiting the negative labor market effects of this type of programs.

Briefly, the main findings are that contemporaneous exposure to SP has no impact on the ratio of formal to total employed, while exposure for at least three quarters leads to a small but statistically significant reduction of 0.78 percentage points or a 4.1 percent decrease in the baseline rate. Furthermore, estimates that account for spillover effects are statistically undistinguishable from those previously derived, suggesting that at average levels of indirect exposure estimates of program impact are not considerably biased. Finally, estimates that explore the effect of direct and indirect exposure at the highest levels

⁵While not directly comparable the CBO (2012) estimates of the affordable health care act suggest that this intervention will extend coverage to 27 million individuals over the next 7 years.

⁶Knaul et al. (2006), Gakidou et al. (2007), Knox (2008), Barros (2008), and King et al. (2009).

⁷A municipality is a second level administrative unit, equivalent to a county in the US.

⁸Campos-Vázquez and Knox (2010), Bosch and Campos-Vázquez (2010), Aterido et al. (2011), and Azuara and Marinescu (2011)

observed in the sample, indicate that the total program effect is only moderately larger (1.5 percentage points).

The paper is organized as follows: Section 2, provides the background on SP. Section 3, describes the identification strategy. Section 4, presents the main results. Section 5, explores the role of spillover effects, and section 6 concludes.

2 Background

From its onset in 1943 the health care system in Mexico has been segmented by employment status. The majority of private sector workers (and their families) are affiliated with IMSS⁹ while public sector employees access their services through ISSSTE.¹⁰ These organizations serve as both social security funds and as health providers. In the case of Mexico, affiliates have no guaranteed package of drugs or interventions, they are only able to access hospitals operated by their own fund, and other types benefits, such as pensions or disability benefits, are bundled with health services.¹¹ In exchange for these benefits employers and employees pay a pay-roll tax premium that is subsidized by the government. Workers contributions roughly correspond to 31.5 % of wages, with almost a third of that amount going to health care.

Since the 1990's Mexicans without formal employment could potentially join IMSS. However, the lack of coverage for pre-existent conditions and the requirement to pay both the employee and the employer contributions meant that inscriptions through this channel are for all purposes negligible. Private insurance while available is used by only a very small share of the population.

Prior to the introduction of SP, those not covered by contributory social security, roughly 66% of the employed,¹² relied on health services provided by clinics and hospitals of the ministry of health. Qualitative assessments have usually pointed out that the services in this network can best be described as "limited, frequently unavailable and often requiring out-of-pocket payments at the point of service", Lakin (2010). However, it is hard to draw general conclusions given that quality varied greatly across states, both as a result of the 1980's decentralization process, and also because of the skewed distribution of federal health funds which tended to favor states that were generally better off, Lakin (2010). That said, given the size of the disparities it is likely that even in the most efficient and well funded states non-contributory social security services were consistently out performed, OECD (2005) and Gakidou et al. (2007).

Following a pilot phase, that took place between the fourth quarter of 2002 and the fourth quarter of 2003, SP was introduced on January 2004. SP represented a departure from the status-quo, because it achieved three broad objectives. First, it substantially increased federal financing to the non-contributory tier, which grew from US \$2.8 billion in 2000 to US \$5.8 billion in 2007, Lakin (2010). Second, it standardized the quality of services by progressively shifting federal resources to states that tended to be under-financed while exerting additional federal control, this was done by attaching conditions to the use of these new resources. Third, it improved efficiency by creating a national risk pool for particularly expensive interventions.

On the whole, for the uninsured population these changes lead to an overall increase in the access to health services, both as consequence of improved infrastructure but also because of the elimination of

⁹By its acronym in Spanish: Instituto Mexicano de Seguro Social

¹⁰By its acronym in Spanish: Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado

¹¹A more extensive list of services include: life insurance, retirement pensions, disability benefits, housing loans, severance payments and in kind transfers such as sports, cultural facilities and day care services.

¹²Calculations from the 2002 Q4 INEGI ENE LFS.

fees at the point of service. Additionally SP increased the scope of health services by introducing for the first time a guaranteed package of services. It originally included 169 interventions, which go from routine check-ups to third level surgeries, as well as 333 drugs deemed capable of covering 90% of the disease burden. Over time the package was continuously upgraded, 269 medical interventions by 2006 and 275 by 2010.¹³ In addition, to this basic package, affiliates are covered through the catastrophic expenditure fund against illness such as AIDS, childhood cancers, cervical cancer, premature birth and other particularly expensive conditions.

Mexican residents 18 years or older not covered by contributory social security and their families, are eligible to receive SP. In order to access the package of services, the head of the household visits an affiliation module, usually located in the primary medical unit of the municipality, there he provides basic documentation, including an identification document know as the CURP, proof of residency and the birth certificate of each of the family members of that is to be covered by the program.¹⁴

During the visit to the affiliation module individuals are also administered a small income evaluation survey, that would be used to determine the premium that families are required to pay for a SP policy. The original design envisioned providing free services to the first two income deciles while individuals in higher income deciles would pay a progressive premium. In practice, the program provided services free of cost for the overwhelming majority of affiliates, [Scott \(2006\)](#). This is probably due to the fact that the survey could be easily manipulated by “field officers trying to meet affiliation targets and by families trying to avoid premium payments”, [Lakin \(2010\)](#), and/or to collective affiliations made by unions, NGO’s and government agencies that were generally exempted from any kind of verification procedure. All in all, while SP may have been envisioned as a health insurance program in practice it operated as a large expansion of non-contributory health care services.

Unfortunately administrative records of when and where SP services had been offered are not available, instead I rely on the number of affiliations by municipality and quarter. In order to recover the sequence in which SP was introduced from this records, I define that a municipality has direct access to SP when the number of affiliations is larger than 10. This cut-off point while arbitrary has no bearing on any of the results of this paper which have been reproduced using various cut-off points (e.g., when the affiliation threshold is set at 5 or 1, available upon request).

Using the previous definition, [Table 1a](#) below, illustrates how the roll out of the program brakes down by year, while [figure 1b](#) highlights that even though the uninsured have the option to affiliate to SP at any point, program take up grew side by side with program roll-out in the post pilot phase. Note that 61% of municipalities received the program between 2004 and 2006 and that during this period SP coverage of the uninsured population sharply increased from 3.6% to 26%. Among those municipalities with direct access to SP the average take up rate of the uninsured population during this same period was in the order of 27%, with municipalities in the top decile reaching coverage rates of up to 88% as detailed in [figure 3](#) of the appendix.

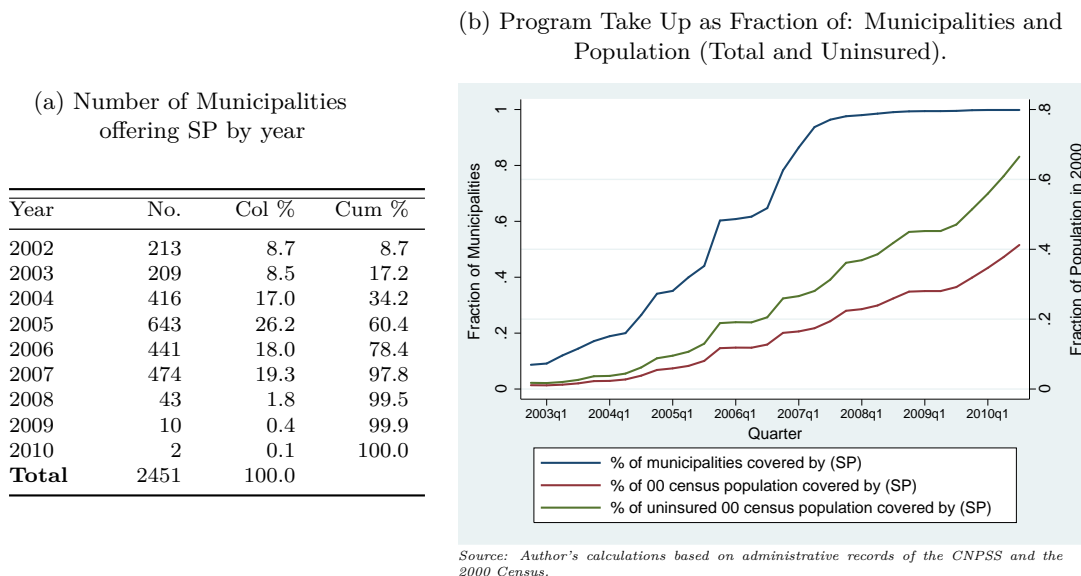
Finally, a detailed description of the variables and the datasets can be found in the appendix. Briefly, this paper employs data produced by the Mexican bureau of statistics (INEGI), including the labor force surveys ENE and ENOE, the 2000 census, geo-statistical datasets at the locality, municipal and state level, as well as municipal level records of public finances and health infrastructure. Additional, data on SP affiliations by municipality and quarter comes from the health and social protection bureau

¹³Annual reports by the Comision Nacional de Proteccion Social en Salud.

¹⁴According to the rules of the program, the spouse or partner, sons, biological or adopted until the age of 18 or up to 25 if in school and unmarried, as well as relatives older than 64 who reside in the same dwelling, will be covered by the issued policy.

(CNPSS). Electoral results and measures of political competition at the municipal and state level have been originally compiled by the financial group (BANAMEX) and the think tank (CIDAC) but have been later on complemented and updated by myself from various public sources.

Figure 1: Geographical Coverage and Program Take up.



3 Identification Strategy

In order to establish the causal impact of SP on formal employment in municipalities that received the program (i.e., the average impact of treatment on the treated), this paper employs a difference-in-differences design that takes advantage of the variation created in space and time by the roll out of the program. Specifically, I compare the change in the ratio of formal to total employment (before and after the introduction of the program) between a group of municipalities that received the program at an early stage (the treatment group) and those that received it later on (the control group).

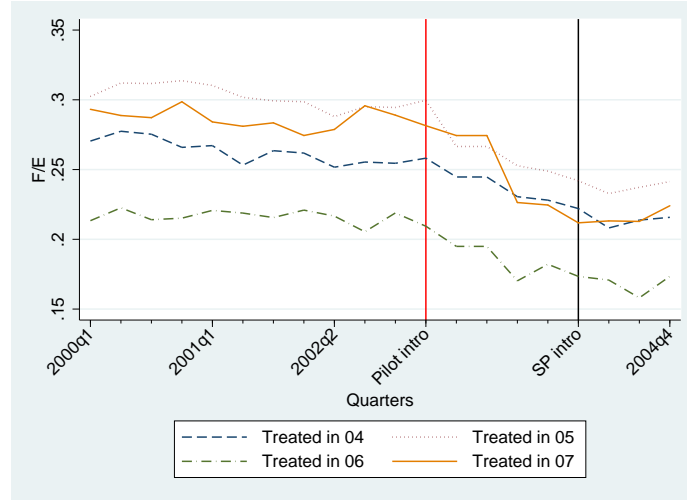
The intuition behind this type of design, is that the change in the treatment group allows me to control for time invariant characteristics of municipalities that may be correlated with the timing of SP and formal employment, while the change in the control group accounts for time varying factors that are common to both control and treatment municipalities. Thus, as long as it can be claimed that the change in the control group provides an unbiased estimate of the counterfactual, it is possible to establish a causal link between SP and its impact on the ratio of formal to total employment. This identification assumption cannot be directly tested but supporting evidence can be provided.

3.1 Pre-Intervention Time Trends

Given that SP services were progressively rolled out across municipalities during the 2002-2007 period there are many potential configurations of the treatment and the control group to exploit. And while the background discussion suggests that it is reasonable to focus on the post 2004 period when the sharp increase in program take up begins, a stronger claim of internal validity can be made by narrowing down the sample of municipalities to those for whom pre-intervention time trends are parallel.

Figure 2 below, provides a first glimpse of how the time trends in the ratio of formal to total employment vary according to the year in which the program was introduced. As can be seen while the time trends between those municipalities that received SP between 2004 and 2006 appear to be parallel, those of municipalities that received the program in 2007 are clearly distinct.

Figure 2: Formal employment trends in the pre-intervention period by year in which the municipalities received SP.



Source: Author's calculations based on ENE.

In order to test whether pre-intervention time trends are parallel between a first obvious choice of groups, namely, municipalities that receive the program in 2005 (treatment) and those that receive it in 2006 (control). I estimate a simple model that uses only pre-program observations (before the first quarter of 2004). The dependent variable is the ratio of formal to total employment in municipality (m) at quarter (t). On the right hand side the model includes an indicator variable equal to 1 if a given municipality receives SP in 2005 and 0 if it did in 2006, as well as a full set of quarter dummies and their interactions with the program indicator variable.

Figure 4b in the appendix, depicts the trends between these two groups which appear to be parallel. This is confirmed by the finding that the coefficients on the interaction terms between the quarter effect and the program indicator variable, are neither individually nor jointly significant at conventional levels (p-value reported in the graph).

Additionally, since figure 2 illustrates that the municipalities that received the program in 2004 also seem to be parallel to both groups, it might be feasible to expand the sample of treated municipalities with those that received the program at this earlier stage. In order to assess whether this is possible, I test whether the pre-intervention time trends between the enlarged treatment group (now including municipalities with SP in 2004) and the control group remain parallel. Figures 4c to 4e in the appendix present these trends when the treatment group is allowed to incrementally include municipalities that received SP at different quarters in 2004. The main finding is that in all cases I am unable to reject the null hypothesis, that pre-intervention quarter dummies are the same between the treatment and the control group. Thereby, suggesting at least five possible configurations of the treatment group that are parallel to the control (municipalities with SP in 2006).

3.2 What factors determined the order in which municipalities received Seguro Popular ?

Program placement was driven by negotiations between the state and the federal government.¹⁵ Once agreements of participation were signed, states would receive federal funds for SP in exchange for following operational guidelines that gave priority to the poorest municipalities. For purposes of identification, targeting on the baseline characteristics of municipalities does not represent a threat to internal validity. However, given that state governors could have had enough leeway to factor in other considerations, Bosch and Campos-Vázquez (2010), it is important to address whether time varying unobserved covariates correlated with both the introduction of SP and formal employment could potentially lead to biased estimates of program impact.

Two alternative objectives of state governments are of some concern. The first is that if constituencies are more demanding of elected officials when they experience shocks, as suggested by a number of behavioral models, then state governments would have an incentive to respond to local economic downturns by pushing for the early implementation of SP.

The second, is that state governments preoccupied with gaining or protecting an electoral edge, may have deployed SP in conjunction with other government programs and/or regulations that affect formal employment. For example, the provision of SP could be accompanied with a simultaneous increase in public sector jobs.¹⁶ Alternatively, it could also be the case that municipal governments controlled by political parties, that have a low tolerance for informal employment, react to the introduction of SP with legal measures that are likely to crack down on informal workers (e.g., city ordinances that regulate street trading).

In order to deal with these concerns the difference-in-differences specification that will be described in detail in the next section, includes a set of time varying controls that directly addresses each of these possibilities.

Additional details on the factors that determined the roll-out sequence as well as a model that predicts SP implementation as a function of pre-program characteristics can be found in the appendix. Briefly, the main finding, is that the economic characteristics of municipalities are uncorrelated with program placement, for the group of municipalities that received the program between 2004 and 2006. This is important because even in the case in which the impact of SP on the labor market varied as a function of these characteristics, estimates of program impact would not be biased as a result of having no comparable municipalities in the control group or vice-versa.

3.3 The difference-in-differences specification.

In terms of the specification of the model, the arguments previously outlined suggest a very particular choice of treatment and control group, and consequently of period of analysis. Figure 5 of the appendix describes in detail the strategy. Ideally, the preferred specification would use municipalities that received SP between 2004 and 2005 as the treatment group and those that received the program in 2006 as the control.

Accordingly, the period under analysis should extend from the first quarter of 2004 till the fourth quarter of 2005. In the case of specifications that use a lagged definition of SP introduction, this basic strategy

¹⁵See Lakin (2010) for a detailed discussion of this process.

¹⁶State and municipal governments are responsible for the provision of: electricity, water, drainage, security, education, and the maintenance of public areas.

has to be adjusted in order to account for the fact that some of the original treatment municipalities (i.e., exposed to SP between 2004 and 2005) would have no with-in variation in this period of analysis.¹⁷ This could be accomplished either by dropping these municipalities from the treatment group or by extending the period of analysis by the number of lags being tested (this latter alternative is equivalent to comparing municipalities exposed to SP for a period longer than the number of lags being tested with municipalities exposed for a shorter period of time.)

For example, in the one lag specification, I can either extend the period of analysis by one quarter (till the first quarter of 2006), thus allowing me to retain in the treatment group municipalities that received SP in the fourth quarter of 2005, or I can use the original period of analysis and drop the municipalities received SP in the fourth quarter of 2005.

In order to maximize statistical power I have chosen to extend the period of analysis in accordance to the number of lags being tested. However, since this implies that some municipalities in the control group could be exposed to SP for a shorter period of time than the the numbers of lags being tested, in section 4 it will be shown, that narrowing down the control group to municipalities that have never been exposed to SP, or dropping treatment group municipalities with no with-in variation and using the original period of analysis, leads to estimates of program impact of a very similar magnitude, albeit noisier given the smaller sample size.

Another element that must be take into account is that Mexican labor force surveys (LFS) underwent a considerable transformation during this period. More specifically, in the first quarter of 2005 the ENOE LFS was introduced, and while its substantial revisions to methodology and variable definitions made it far more suitable for international comparisons than the previous ENE LFS,¹⁸ it created a host of problems for drawing comparisons across surveys, see INEGI (2009) for a review of the limitations. In the case of this paper, revisions in variable definitions are of particular concern, as these changes could lead to biased estimates of program impact.¹⁹

In order to avoid this potential problems, the main results of this paper will be derived using only data and variable definitions from the new LFS ENOE. This choice comes at a relatively small cost as I will still be able to use most of variation created by program roll out in 2004. Figure 6 of the appendix provides the details for each specification. Succinctly, the main implication of this data constrain is that the period of analysis will now necessarily have to start in the first quarter of 2005, and consequently, the treatment group has to be narrowed down to those municipalities for whom there is with-in variation in their respective periods of analysis.²⁰ For example, in the one lag specification, municipalities that received SP in 2004 will not be included as they no longer contribute to identifying the impact of the program.

Note, however, that specifications that test higher order lags will progressively allow the introduction of municipalities exposed to SP in 2004 into the treatment group, as these group will have with-in variation in the post 2005 period. For example, the two lag specification, can now additionally include municipalities

¹⁷If in the fourth quarter of 2005 a municipality has been exposed to SP for a number of quarters smaller or equal than the number of lags being tested, the SP indicator variable will be zero throughout the period of analysis

¹⁸This LFS was produced by INEGI between the first quarter of 2004 and the fourth quarter of 2004.

¹⁹For example, while the ENE considers as employed absent workers without a labor contract (e.g. self employed who worked less than one hour or did not earn from this activity in the reference week) the ENOE does not. If I take the relevant definition to be that of the ENE, and assume that some of these workers are absent as a consequence of health shocks that could have been prevented with SP, or conversely that they are more likely to be absent as a result of being diagnosed. Then it is reasonable to expect the measurement error in the ratio of formal to total employment derived from ENOE surveys will be (negatively/positively) correlated to SP implementation, potentially leading to a (downward/upward) bias in the estimates of program impact.

²⁰If in the first quarter of 2005 a municipality has been exposed to SP for a number quarters greater or equal to the number of lags being tested, the SP indicator variable will be one through out the period of analysis.

that received the program in the fourth quarter of 2004.

This changes in the composition of the treatment group along with the previous discussion on the appropriate period of analysis explain the differences in sample size between each of the specifications that will be tested. Bear in mind that pre-intervention time trends have been shown to be parallel for each of these configurations.

Another important specification choice was that of performing the analysis at the municipal level. This decision is partly motivated by the fact that program implementation took place at this level, and more fundamentally, because this type of specification provides a natural way of deriving an upper-bound of program impact, when the effect of SP is expected to be driven by smaller municipalities (in terms of population), Bosch and Campos-Vázquez (2010). That said, given that controls might perform better at the individual level, section 4 verifies that results at the municipal and individual level are consistent.

Formally the difference-in-differences model can be specified as a two-way fixed-effect model, the basic setup is given by equation 1 below.

$$\frac{F_{mt}}{E_{mt}} = \alpha + \beta_t + \beta_m + \beta_1 SP_{m,t-L} + \omega X_{mt} + \epsilon_{mt}, \forall m \in M, t \in T \quad (1)$$

Where the subsample of municipalities (M) and the period of analysis (T) are as previously discussed and as illustrated in figure 6 of the appendix. A detailed description of variable definitions and their sources can be found in section A.1 of the appendix. Briefly, the left hand side variable $\frac{F_{mt}}{E_{mt}}$ is the ratio of formal to total employment in municipality (m) at quarter (t). On the right hand side, $SP_{m,t-L}$ is a dummy variable that takes the value one if SP is being offered in municipality (m) at quarter (t-L) and is zero otherwise, L represents the number of lags that is being tested, $\{L \in \mathbb{Z} | 0 \leq x \leq 4\}$. β_m is a fixed effect unique to municipality (m) and β_t is a time effect common to all municipalities in quarter (t). X_{mt} is a vector of control variables that vary across municipalities and time.

Unless otherwise stated the vector X_{mt} is composed of three sets of time varying controls. The first group, accounts for changes in the demographic composition of municipalities, it includes employed population shares of: age (5 groups), educational attainment (4 groups), marital status (6 groups), gender and urban status.

The second group, address the possibility that program placement responded to economic shocks, it includes employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality.

The third group, controls for both the possibility that SP may have been introduced in conjunction or in replacement of other government programs, and that different political parties (while in office) may have pursued policies and regulations capable of affecting formal employment, it includes: total and infrastructure expenditures per capita at the municipal level, a set of municipal level dummies that take the value of 1 when one of the three main political parties or their alliances (eight groups in total) holds a Mayor's post, and an analogous set of state level dummies for the political affiliation of the governor.

4 The impact of SP on formal employment.

This section tests whether contemporaneous or lagged exposure to SP (up to 4 quarters) affects the ratio of formal to total employment. The OLS estimation results of equation 1 for different degrees of exposure are presented in table 2, columns 1 to 5. First, I find no evidence of SP having a contemporaneous effect on the share of formal employed, column 1. Second, exposure for at least three quarters leads to a small but statistically significant reduction of 0.78 percentage points, this amounts to a 4.1 percent reduction of the baseline rate,²¹ column 4. Third, exposure to SP for at least four quarters is associated with a 0.7 percentage point reduction in the ratio of formal to total employment, column 5.

In all cases I am able to sharply estimate the impact of the program. Minimum detectable effect (MDE)²² calculations, presented on the last row of table 2, suggest that if testing could be carried out recurrently, in 80% of the cases I would be able to reject the null hypothesis of no program effect at the five percent level, as long as the impact of SP on the ratio of formal to total employment was in the order of 1.5 to 1.1 percentage points.

This latter result, however, depends on the assumptions that have been made regarding the term ϵ_{mt} of equation 1, which represents a municipal time varying error that is assumed to be independently distributed of β_m and β_t . In order to account for the possibility that the error term is correlated across time, for example, because of persistence in regional shocks to labor demand or supply, I allow for an arbitrary covariance structure within municipalities and over time by computing standard errors clustered at the municipal level.

Additionally, since it is also reasonable to suspect that the error term could be correlated through space, standard errors clustered at the state-quarter level are presented in brackets. Last, I provide the most conservative estimates of standard errors by following the work of Cameron et al. (2011) on multi-way clustering which allows for simultaneous clustering at the municipal and state-quarter level, this type of standard errors are reported in crochets. As can be seen from the comparison of these estimates, the main finding is that neither the statistical significance of the estimates of program impact, nor the reliability of the MDE calculations hinge on the assumptions that are made with respect to the standard errors.

In order to further ascertain whether the estimated impact of SP on formal employment is robust, tables 3 and 4 below, presents a number of variations to the specification in which the strongest case for any impact of SP on the labor market can be made, namely, exposure for at least three quarters. These variations and robustness checks can be categorized in 6 groups.

First, columns 1 to 4 of table 3 emphasize the importance of controlling for time varying factors capable of confounding the impact of SP. The main finding is that although the coefficients are statistically indistinguishable from each other across columns, controlling for these factors is important as it leads to different interpretations. More specifically, this can be seen by comparing the coefficient with no controls in column 1 to any of the other coefficients. While in the former a case for a zero result could be made, in the latter the larger and statistically significant coefficients suggest that SP was in fact capable of reducing formal employment albeit only slightly.

Second, columns 5 to 11 expand the benchmark specification (i.e. column 4) by including a series of additional controls. More specifically, in column 5, it is corroborated that the empirical strategy is

²¹The average ratio of formal to total employment in control municipalities in the first quarter of 2005 is 0.187.

²²The MDE is defined as $(t_{\alpha/2} + t_{1-\kappa})\sigma_{\hat{\beta}}$, if $\alpha = 0.05$ and $\kappa = 0.8$ is assumed, $MDE \approx 2.8\sigma_{\hat{\beta}}$. In the MDE calculations of table 2 standard errors clustered at the municipal level are used.

appropriately accounting for time varying factors by estimating a very demanding specification that includes both the contemporaneous and the lagged vector of controls, that is, $X_{m,t}$ and $X_{m,t-L}$ as can be seen, the estimate of program impact varies only slightly.

Table 2: Current and Lagged Effect of SP

VARIABLES	(1) F/E	(2) F/E	(3) F/E	(4) F/E	(5) F/E
SP (=1)	0.0015 (.0055) [.0048] {.005}				
Lag SP (=1)		-0.0054 (.0055) [.0058] {.0058}			
Lag 2 SP (=1)			-0.0035 (.0045) [.0049] {.0049}		
Lag 3 SP (=1)				-0.0078 (.0039)** [.0038]** {.004}*	
Lag 4 SP (=1)					-0.0069 (.0038)* [.0035]** {.0038}*
Observations	1,505	1,949	2,775	3,698	4,297
Number of Municipalities	380	413	492	561	573
Num State x Quarter Clusters	96	120	150	175	208
MDE	.0153	.0153	.0127	.0108	.0107

Note: OLS estimation. Robust standard errors clustered at the municipal level in parentheses, at the state-quarter level in brackets, and in both dimensions through multi-way clustering in crochets, * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the ratio of formal to total employment. SP is a dummy variable that takes the value of 1 once a SP facility is open and registers at least 10 affiliations in a given municipality and quarter. All regression include municipal and quarter fixed effects as well as a set of time varying controls organized in three categories. Controls I, include: employed population shares of age (5 groups), educational attainment (4 groups), marital status (6 groups), urban and gender. Controls II, include: employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality. Controls III, include: total and infrastructure expenditure per capita, as well as a set of municipal and state level dummies that take the value of 1 when one of the 3 main political parties or their alliances (8 groups in total) holds a Mayor or Governor post. The subsample of municipalities (M) and the period of analysis (T) used in each regression is as described in section 3.3. *The ratio of formal to total employment in control municipalities at baseline (05q1) is 18.7%.*

Next in columns 6 and 7, it is assessed whether the impact of SP could be underestimated as result of

not taking into account the strategic response of contributory social security providers, (e.g., IMSS could have responded to increased competition by increasing the quality and coverage of services offered to its affiliates). Column 6 includes municipal level, contributory social security medical personnel per capita as a control, while column 7 uses the number of medical units per capita. As can be seen in both cases the coefficients are statistically significant at conventional levels and are of a very similar magnitude to the benchmark specification.²³

Additionally, in order to control for changes in the characteristics of employers and the composition of local labor markets, the specification in column 9, includes employed population shares of the size of firms and the type of industry. As before, the estimate of program effect remains statistically significant and is of a similar magnitude.

More generally, the specifications in column 9 and 10, assess whether unobserved time varying factors that affect regions²⁴ or states equally are capable of driving the results this is done by including region-quarter and state quarter fixed effects. As can be seen, the introduction of region-quarter dummies has no bearing on the results, while the demanding state-quarter fixed effect specification²⁵ leads to a smaller SP coefficient (5.8 percentage points) that is not statistically significant at conventional levels. Since changes in the level of economic activity at the state level are a primary source of concern, column 11, introduces a state level electricity consumption index as control. In this case the estimates of program impact are undistinguishable from those of the benchmark specification.

The other types of robustness checks, are presented in table 4. Specifically the third group, columns 1 and 2, assess whether a nonlinear panel data model that recognizes the bounded nature of the dependent variable is able to provide a better approximation to the conditional expectation function. Following Papke and Wooldridge (2008), I employ a pooled fractional probit (PFP) in order to recover the average partial effect of SP. The coefficients are estimated using quasi-maximum likelihood,²⁶ while the standard errors are derived by bootstrapping. Since the method is only defined for the case of a balanced panel, for purposes of comparison, column 1, presents the OLS fixed effects estimate for this case, (0.72 percentage points). Reassuringly, the (PFP) estimate of program impact is only slightly bigger (0.75 percentage points).

The fourth group addresses an issue previously discussed, namely, that of ruling out that the estimates of program impact could be under-estimated by the exposure of some control group municipalities to SP. In the benchmark specification, the period analysis runs from the first quarter of 2005 till the third quarter of 2006. Accordingly, the treatment group is composed by the municipalities that received SP between the third quarter of 2004 and the fourth quarter of 2005, while the control group is composed of those municipalities that received the program in 2006. This implies that I am effectively comparing municipalities that have been exposed to SP between 4 and 9 quarters with those that have been exposed between 0 and 3.

In order to show that the results are not being driven by changes in the control group, the specification on column 3, restricts control group municipalities to those that received SP in the fourth quarter of 2006 (i.e., those that have never been exposed to SP). Additionally, the specification in column 4, narrows down the period of analysis (first to fourth quarter of 2005) and drops treatment municipalities that have no with-in

²³Very similar results are obtained when total medical personnel and units are used as controls, available upon request.

²⁴The definition of region that is used comes from the work of Aroca et al. (2005) who identifies three regions with very distinct economic performance. The first region is composed by those states that have a common border with the United States (Baja California, Chihuahua, Coahuila, Nuevo Leon, Sonora and Tamaulipas), the second roughly correspond to states in the south (Chiapas, Guerrero, Oaxaca, Puebla, Tlaxcala, Veracruz, Yucatan and Quintana Roo) while the third is made up of the remaining central states.

²⁵This specification requires the estimation of 134 additional coefficients.

²⁶This specification includes time averages of each regressor instead of municipal fixed effects.

variation in this time frame, that is to say, I am now comparing treatment municipalities exposed to SP between 3 and 5 quarters to the full control group (never exposed given the time frame). Reassuringly, both coefficients are of a similar magnitude to those derived under the benchmark specification, albeit noisier given the smaller sample size. Note additionally, that the specification on column 4, allows me to also rule out that the effect of SP is being driven by treatment municipalities with very prolonged exposure 6 to 9 quarters.

The fifth group of robustness checks deals with the definition of the dependent variable. More precisely, in column 5, I take into account that coverage to contributory social security can be gained indirectly (e.g. through a spouse that has formal employment), by using a broader definition in which any employed member of a household where at least one member has access to contributory social security will be counted as formally employed.²⁷ As can be seen in column 5, while the coefficient is somewhat larger it is statistically indistinguishable from the one derived from the benchmark model.

Additionally, in order to test whether the loss in information due to aggregation at the municipal level has caused any problems, column 6, presents an analogous individual level specification. The dependent variable is a dummy that take the value of one when an individual is formally employed, the regressors include: a SP indicator variable, municipal and quarter fixed effects, and the same set of controls (defined at the individual level whenever possible). The model is estimated by OLS using sample weights adjusted to give equal weight to every municipality. As can be seen, the estimated program effect is statistically significant and slightly larger than the benchmark specification. When unadjusted sample weights are used, a similar, albeit smaller coefficient (0.57 percentage points, p-val 0.126), is recovered.

Another possible source of concern is that the (LFS) was not designed to be representative of each municipality to address this issue the specification on column 7, restricts the sample to municipalities on the top 2/3 of the ratio of (LFS) interviews to total employed population derived from the 2000 census. Alternatively, the specification on column 8, assumes that smaller municipalities are noisier and restricts the sample to those with a population larger than 2500 inhabitants as determined by the 2000 census. In both cases, while the estimates are noisier given the smaller sample size, their magnitude is very similar to that of the benchmark specification.

Finally, In order to bolster the case for a causal interpretation of the estimates of program impact, columns 9 and 10 present a falsification exercise were information on the pre-program period (fourth quarter of 2003 to fourth quarter of 2001) is used to construct a placebo. More precisely, the specification on column 9, tests the effect of SP on placebo data while, the specification in column 10, replicates the benchmark specification restricting the sample to those municipalities that can be observed at both points in time (roughly 1/3 of the sample).

Reassuringly, the placebo specification has the wrong expected sign, while the coefficient in column 10 has the correct sign and is of a similar magnitude as the benchmark specification.²⁸ The two coefficients are not statistically different from each other at conventional levels (pval-0.164) but this result has to be interpreted taking into account that sample size is much smaller in this exercise.

²⁷These members of the household who indirectly receive access, must also comply with the eligibility criteria set by contributory social security

²⁸The two specifications in column 9 and 10 were jointly estimated using SURE, then a χ^2 test was performed, the null hypothesis is that the two coefficients are equal.

Table 3: Robustness Checks

VARIABLES	Controls in Steps				Additional Controls						
	(1) F/E	(2) F/E	(3) F/E	(4) F/E	(5) F/E	(6) F/E	(7) F/E	(8) F/E	(9) F/E	(10) F/E	(11) F/E
Lag 3 SP (=1)	-0.00462 (0.00399)	-0.00625* (0.00365)	-0.00792** (0.00355)	-0.00777** (0.00386)	-0.00790* (0.00457)	-0.00802** (0.00386)	-0.00675* (0.00376)	-0.00655** (0.00319)	-0.00709* (0.00406)	-0.00579 (0.00441)	-0.00778** (0.00386)
	0.247	0.0876	0.0261	0.0447	0.0847	0.0380	0.0732	0.0403	0.0814	0.190	0.0446
Observations	4,176	4,176	4,174	3,698	2,748	3,698	3,698	3,698	3,698	3,698	3,698
No. of municipalities	608	608	608	561	491	561	561	561	561	561	561
Municipal & Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls I	.	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls II	.	.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls III	.	.	.	✓	✓	✓	✓	✓	✓	✓	✓
L. Controls I-III	✓
Controls IV	✓
Controls V	✓
Controls VI	✓	.	.	.
Controls VII	✓
Region*Quarter FE	✓	.	.
State*Quarter FE	✓	.

Note: OLS estimation. Robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the ratio of formal to total employment. SP is a dummy variable that takes the value of 1 once a SP facility is open and registers at least 10 affiliations in a given municipality and quarter. Controls I: employed population shares of age (5 groups), educational attainment (4 groups), marital status (6 groups), urban and gender. Controls II: employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality. Controls III: total and infrastructure expenditure per capita, as well as a set of municipal and state level dummies that take the value of 1 when one of the 3 main political parties or their alliances (8 groups in total) holds a Mayor or Governor post. Controls IV: Medical personnel per capita provided by contributory social security. Controls V: Medical units per capita provided by contributory social security. Controls VI: employed population shares of industry (12 groups) and size of firms (7 groups) Controls VII: An index of electricity consumption at the state level.

Table 4: Robustness Checks II

	Balanced Panel	PFP (QMLE)	Robust Control Group	HH Definition	Individual Lvl.	Representativeness MunAvg.		Placebo		
VARIABLES	(1) F/E	(2) F/E	(3) F/E	(4) F/E	(5) F/E	(6) F/E	(7) F/E	(8) F/E	(9) Placebo	(10) F/E
Lag 3 SP(=1)	-0.00716* (0.00400) 0.0742	-0.00749* [.0043] 0.0795	-0.00601 (0.00404) 0.137	-0.00826 (0.00628) 0.189	-0.00982* (0.00521) 0.0601	-0.00931** (0.00374) 0.0132	-0.00752 (0.00474) 0.113	-0.00715* (0.00392) 0.0686	0.00364 (0.00387) 0.346	-0.00473 (0.00493) 0.337
Observations	3,451	3,451	3,372	1,048	3,698	463,673	2,412	3,602	1,693	1,693
No. of Municipalities	493	493	513	264	561	561	411	544	325	325
χ^2										1.937
Prob > χ^2										0.164

Note: OLS estimation unless stated otherwise. Robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the ratio of formal to total employment. SP is a dummy variable that takes the value of 1 once a SP facility is open and registers at least 10 affiliations in a given municipality and quarter. All regression include municipal and quarter fixed effects as well as a set of time varying controls organized in three categories. Controls I, include: employed population shares of age (5 groups), educational attainment (4 groups), marital status (6 groups), urban and gender. Controls II, include: employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality. Controls III, include: total and infrastructure expenditure per capita, as well as a set of municipal and state level dummies that take the value of 1 when one of the 3 main political parties or their alliances (8 groups in total) holds a Mayor or Governor post. Column 2, presents a pooled fractional probit model estimated using quasi-maximum likelihood, this specification includes time averages of each regressor instead of municipal fixed effects, the coefficient corresponds to the average partial effect of SP, bootstrap standard errors in brackets. Column 3, restricts the control group to those municipalities that have not been exposed to SP at any point in time. In column 4, The period of analysis spans from the first quarter of 2005 till the fourth quarter of 2005, accordingly treatment group municipalities with no with-in variation are dropped from the sample. In column 5, The ratio of formal to total employed is calculated assuming that all employed members of a household are covered by contributory social security when at least one of the members declares to have access. Column 6, estimates the analogous individual level model, the regression is estimated using sample weights adjusted to give equal weight to every municipality. Column 7, Restricts the sample to municipalities in the top 2/3 of the ratio of interviews to total employment. Column 8, Restricts the sample to municipalities with population over 2500. The placebo specification columns 9 and 10, restricts the sample to municipalities observed at both points in time, the ratio of formal to total employment in column 9 is calculated using pre-program observations (03q4-01q4).

5 Accounting for Spillover Effects

There are three potential mechanisms through which the introduction of SP could have affected neighboring municipalities. The first is that the eligibility conditions of SP did not restrict the services to residents of the municipalities where the program was introduced. The second, is that if the introduction of SP is viewed as a local labor shock, then as discussed Manning and Petrongolo (2011), its effect is likely to disseminate across overlapping labor markets that could perhaps extend through various municipalities. The third, is that individuals in neighboring municipalities are likely to respond strategically to information that makes them aware of the availability of the program or that helps them gauge in advance the quality of the services that are being implemented.

In this section my focus is not that of disentangling the channels through which spillover effects might be operating, but rather to address these concerns by taking advantage of the idea that the intensity of indirect exposure to SP is likely to be related to the variation in the spatial coverage of the program. More specifically, I use two proxies of indirect exposure to SP in order to establish whether the estimates derived in section 4 are biased as a result of not accounting for spillover effects, and to investigate whether the total impact of SP varies in relation to the degree of indirect exposure.

The first proxy, is road distance from the geometrical center (centroid) of the largest urban area in a municipality to the corresponding centroid of the nearest municipality where SP is being offered in a given quarter.²⁹ The intuition behind this proxy is that as the program becomes denser and the average distance to nearest municipality offering SP is reduced, the more likely it becomes that control group municipalities could have had access to SP. Similarly, this reduction in transactional costs (e.g. transportation costs) should affect treatment and control group municipalities, by decreasing the intensity of formal job search performed by residents of municipalities with direct access to the program, and by increasing the likelihood that individuals will be better informed about the availability and services of SP.

The second is the population weighted share of neighboring municipalities³⁰ with direct access to SP, (PWSN). In this case as the PWSN increases, I expect individuals in control municipalities to be more likely to access SP services and to face less competition for formal jobs as a result of their neighbors access to SP. Additionally, for both treatment and control group municipalities it is reasonable to expect that a higher PWSN should be associated with a higher awareness of SP availability and the quality of its services.

To introduce these proxies into the analysis I modify the difference-in-differences specification presented in equation 1. In the case of distance to the nearest municipality offering SP, equation 2 below, introduces three additional terms: First, $D_{m,t-\lambda}$ which denotes the natural logarithm³¹ of road distance to the nearest municipality offering SP for municipality (m) at quarter ($t - \lambda$), where λ represents the number of lags of indirect exposure to SP $\{\lambda \in \mathbb{Z} | 1 \leq \lambda \leq 4\}$. Note that this lag is independent from lag (L) found on $SP_{m,t-L}$, this additional flexibility is introduced into the model in order to allow the intensity of indirect exposure to vary over time and/or to account for the possibility that the impact of different spillover effects may become relevant at different points in time. Second, an interaction term between $SP_{m,t-L}$ and $D_{m,t-\lambda}$. Third, an interaction between $SP_{m,t-L}$ and the term UD_m , which denotes the natural logarithm of road distance to the nearest municipality, this latter term is introduced in order to

²⁹The largest urban area is defined by population taken from the 2000 census.

³⁰Neighboring municipalities are defined as those that have a common border. Note that the influence of the neighboring municipality does not depend on the length of the border but rather on its population, for further details about the calculation please refer to the appendix.

³¹This transformation is performed in order to minimize the impact of outliers.

control for the fact that $D_{m,t-\lambda}$ could potentially pick up the heterogeneity in the impact of the program in relation to other factors, for example, the area of municipalities.

$$\begin{aligned} \frac{F_{mt}}{E_{mt}} = & \alpha + \beta_t + \beta_m + \beta_1 SP_{m,t-L} + \beta_2 D_{m,t-\lambda} + \beta_3 SP_{m,t-L} * D_{m,t-\lambda} + \beta_4 SP_{m,t-L} * UD_m \\ & + \omega X_{mt} + \epsilon_{mt}, \quad \forall m \in M, t \in T \end{aligned} \quad (2)$$

In analogous manner the specification that uses the second proxy, equation 3 includes instead the terms: $SN_{m,t-\lambda}$ which denotes the population weighted share of neighbors with direct access to SP for municipality (m) at quarter ($t-\lambda$). The interaction term between $SP_{m,t-L}$ and $SN_{m,t-\lambda}$ and the interaction term between $SP_{m,t-L}$ and the term $NPOP_m$, which denotes the natural logarithm of the total population of neighboring municipalities, this latter term is included in order to rule out that the results are driven by program heterogeneity in relation to other factors, such as the size of the labor market of neighboring municipalities.

$$\begin{aligned} \frac{F_{mt}}{E_{mt}} = & \alpha + \beta_t + \beta_m + \beta_1 SP_{m,t-L} + \beta_2 SN_{m,t-\lambda} + \beta_3 SP_{m,t-L} * SN_{m,t-\lambda} + \beta_4 SP_{m,t-L} * NPOP_m \\ & + \omega X_{mt} + \epsilon_{mt}, \quad \forall m \in M, t \in T \end{aligned} \quad (3)$$

The other variables in equations 2 and 3, as well as the sample of municipalities included (M) and the time frame (T) are the same as those of section 4. The intuition behind this new specifications, is that of assessing the impact of SP on formal employment when the total impact of the program is allowed to depend on both direct and indirect exposure to SP. Accordingly, the focus of this paper shifts toward the marginal effect of SP, which in the case of the first proxy, is given by the expression $\beta_1 + \beta_3 * D_{m,t-\lambda} + \beta_4 * UD_m$ or equivalently by β_1 once the variables $D_{m,t-\lambda}$ and UD_m are centered at a distance that is relevant for the analysis.

Since deriving standard errors for this latter alternative is straightforward, for the rest of this section I will focus on β_1 , which represents the impact of SP treatment conditional on the degree of indirect exposure (the value at which the interaction terms are centered). The coefficients $D_{m,t-\lambda}$ in equation 2 and $SN_{m,t-\lambda}$ in equation 3, are also of interest as they allow me to gauge whether indirect exposure to SP mattered in control municipalities. Finally, it is important to note that this analysis is based on an identification assumption that is far more stringent, namely, that pre-program trends between treatment and control municipalities are parallel conditional on the proxy that is being used.

Given that the results of section 4 suggest that the largest reduction in formal employment is associated with an exposure of at least three quarters to SP, table 5 below explores whether this estimate of program impact is biased as a result of spillover effects. This is done by comparing the coefficient of the benchmark model with the ones derived from equations 2 and 3 when the interaction terms are centered at their respective averages. Each panel tests a different proxy of indirect exposure, column 1 reproduces the result from section 4, while columns 2 to 5 investigate the impact for different lags (λ) of indirect exposure to SP.

The main findings, are that regardless of the proxy used, the coefficients of SP impact at average spillover levels are of a very similar magnitude to that of the benchmark model, and that in all cases these

coefficients are statistically undistinguishable³² from the benchmark coefficient.³³ Moreover, consistent with the idea that the results of section 4 are not overestimating the impact of SP, I am also unable to provide any evidence of indirect exposure to SP having an effect on control group municipalities. In order to rule out that these results are specific to municipalities that have been directly exposed to SP for at least three quarters, tables 15 and 16 in the appendix, replicate this analysis for each specification discussed in section 4. The results are analogous in all cases.

In order to provide an upper-bound of program impact, I additionally estimate the impact of SP on municipalities that had direct access to SP and that were additionally indirectly exposed at high intensities. This is done by making two types of assumptions. First, since it is reasonable to conjecture that in most cases the intensity of spillover effects is inversely related to distance to the nearest municipality offering SP, and directly related with the PWSN,³⁴ I calculate the marginal effect of the program at the smallest $D_{m,t}$ and the largest $SN_{m,t}$ observed in the sample.

Second, since this calculation also depends on the timing of direct and indirect exposure to SP, for which I have no strong priors, I employ the most generous assumptions and examine every possible lag combination. This of course implies testing a large number of hypotheses³⁵ and raises the issue of accounting for the false discovery rate (FDR), which I address by calculating both FDR and “sharpened” FDR adjusted p-values, from here on q-values, Anderson (2008).³⁶

Table 17 in the appendix, presents the marginal effect of SP for every possible lag combination when $D_{m,t}$ and UD_m are centered at 10km. Each entry is from a separate OLS regression of equation 2, the panels vary the timing of direct exposure to SP (lag L), while the columns correspond to a different lag of indirect exposure to SP (lag λ). The main finding is presented in panel 3 column 4, where the largest and most sharply estimated total program effect is found (1.5 percentage points), q-values 0.12 and 0.14.³⁷ The results of table 17 further suggest that prolonged indirect exposure to SP matters most in municipalities that have been directly exposed to SP for shorter periods of time, however, in all cases I am unable to reject the null hypothesis of no program effect when controlling FDR at $q = 0.10$.

In analogous manner, table 18 in the appendix, calculates the marginal effect of SP when $SN_{m,t}$ is centered at a share of 0.85 and $NPOP_m$ at the median of the sample.³⁸ Consistent with the previous set of results, the point estimate of panel 3 column 4, suggests a program effect in the order of (1.4 percentage points), q-values 0.15 and 0.17,³⁹ while the estimates of panels (1-3) in columns 4 and 5 lend additional support to the idea that prolonged exposure matters most when it precedes direct exposure,⁴⁰ as in the previous case no result rejects when controlling FDR at $q = 0.10$.

Finally, in order to test the sensitivity of these upper-bounds to the degree of indirect exposure, table 19 and 20 in the appendix, calculate the marginal effect of SP holding the choice of lags constant while allowing the proxies of indirect exposure to vary ($D_{m,t}$ 7-100 km, $SN_{m,t}$ 0.95-0.25). The main finding is that in both cases the slightly larger program effects are relevant for less than 1/5 of the municipal-quarter

³²This is accomplished by jointly estimating the different specifications using SUR and then performing χ^2 tests where the null hypothesis is that the coefficients are equal.

³³Very similar results are obtained when both of these proxies are used simultaneously (i.e., by including in equation 3 the term $\beta_3 SP_{m,t-L} * SN_{m,t-\lambda} * D_{m,t-\lambda}$ in addition to all the secondary terms), available upon request.

³⁴This is not necessarily the case for the labor market spillover effect.

³⁵There are 25 possible combinations of lags for each proxy.

³⁶The actual calculation is as described in Benjamini and Hochberg (1995) and in Benjamini et al. (2006)

³⁷This result is relevant for 255 municipal quarter-observations, 225 in treatment and 30 in control.

³⁸The median is used because a few municipalities have very large neighbors, that said, centering with the mean of total neighbors population produces similar results.

³⁹This result is relevant for 536 municipal quarter-observations, 505 in treatment and 31 in control.

⁴⁰The results on panel 1 columns 3 and 4 estimates a program effects as large as 2.3 percentage points, however, they are not emphasized because there are only 26 municipal-quarter observations for whom this result is relevant.

observations and that weak indirect exposure (i.e., high $D_{m,t}$, low $SN_{m,t}$) is associated with smaller, but never statistically significant positive effects of SP on formal employment.

Table 5: Estimates of program impact at average levels of indirect exposure.

VARIABLES	(1) F/E	(2) F/E ($\lambda = 1$)	(3) F/E ($\lambda = 2$)	(4) F/E ($\lambda = 3$)	(5) F/E F($\lambda = 4$)
Panel 1					
Lag 3 SP(=1)	-0.0078** (0.0038)	-0.0086 (0.0065)	-0.0055 (0.0069)	-0.0059 (0.0070)	-0.0066 (0.0042)
$D_{m,t-\lambda}$ (C at mean)		0.0021 (0.0035)	0.0014 (0.0031)	0.0036 (0.0029)	0.0025 (0.0033)
$SP_{m,t-3} \times D_{m,t-\lambda}$ (C at mean)		-0.0033 (0.0083)	0.0006 (0.0076)	-0.0025 (0.0067)	-0.0021 (0.0037)
$SP_{m,t-3} \times UD_m$ (C at mean)		0.0077 (0.0088)	0.0041 (0.0081)	0.0060 (0.0070)	0.0059 (0.0040)
Observations	3,698	3,698	3,698	3,698	3,698
Clusters	561	561	561	561	561
χ^2 eq(1)-eq($\lambda+1$)		0.0267	0.150	0.0946	0.473
Prob > χ^2		0.870	0.698	0.758	0.492
Mean $D_{m,t-\lambda}$		42.97	51.08	61.47	72.18
Panel 2					
Lag 3 SP(=1)	-0.0078** (0.0038)	-0.0062 (0.0052)	-0.0056 (0.0051)	-0.0058 (0.0053)	-0.0052 (0.0044)
$SN_{m,t-\lambda}$ (C at mean)		-0.0022 (0.0073)	0.0033 (0.0068)	0.00051 (0.0070)	-0.0012 (0.0088)
$SP_{m,t-3} \times SN_{m,t-\lambda}$ (C at mean)		0.0020 (0.0124)	-0.0018 (0.0110)	-8.23e-05 (0.0110)	-0.0013 (0.0090)
$SP_{m,t-3} \times NPOP_m$ (C at mean)		0.0035 (0.0033)	0.0035 (0.0033)	0.0035 (0.0034)	0.0035 (0.0034)
Observations	3,698	3,698	3,698	3,698	3,698
Clusters	561	561	561	561	561
χ^2 eq(1)-eq($\lambda+1$)		0.179	0.325	0.229	1.201
Prob > χ^2		0.672	0.569	0.632	0.273
Mean $SN_{m,t-\lambda}$		0.604	0.527	0.438	0.349

Note: SUR estimation, robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include municipal and quarter fixed effects as well as time varying controls I-III as described in section 4. The lag 3 of SP coefficient is the marginal effect of the program at average levels of indirect exposure. $D_{m,t-\lambda}$ is the natural logarithm of road distance to the nearest municipality offering SP. UD_m , is natural logarithm of road distance to the nearest municipality. $SN_{m,t-\lambda}$ is the population weighted share of neighbors with direct access to SP. $NPOP_m$, is the natural logarithm of the population of neighboring municipalities. In all cases these covariates have been centered at their respective sample averages.

6 Conclusions

The results of this paper indicate that the large scale provision of free health services to the uninsured had a small distortionary effect on the Mexican labor market. Specifically while I find no evidence of a contemporaneous effect of Seguro Popular on the ratio of formal to total employment, I am able to show that exposure to the program for at least three quarters leads to a small but statistically significant reduction of 0.78 percentage points.

A number of factors support the causal interpretation of these findings. First, the treatment and control groups showed similar trends in the pre-program period. Second, time-varying factors capable of affecting program placement and formal employment have been introduced as controls. Third, robustness checks and a falsification exercise corroborate the validity of the identification strategy. Fourth, estimates of program impact are not biased as a result of spillover effects. Fifth, an upper-bound of program effect, derived under the most favorable assumptions, for municipalities that were directly and indirectly exposed at high intensities is only moderately larger (1.5 percentage points).

Given the size of the estimates of program impact, an important question is establishing the factors that limited the potentially negative impact of the program. In addition to the conventional arguments that these services only represent a small proportion of the overall non-monetary benefits associated with informal jobs, or that the actual improvement in the quality of services over the status quo may have been small, the findings of this paper are broadly consistent with two complementary explanations that merit further research.

The first is that regardless of the actual change in the quality of services perceived improvements, such as shorter waiting times for appointments, are hard to gauge initially. If potential users learn incrementally about the new benefits, through their experience and that of others in their network, then it is reasonable to observe that the impact of the program is lagged and the effect will be stronger in treatment municipalities whose neighbors have also been exposed to the program.

The second explanation is that Seguro Popular is not a default option but a choice that was not accompanied with an appropriate intervention to raise awareness (e.g., in the case of Chile Solidario social workers have been shown to be instrumental in encouraging program take-up Galasso (2011)), thus as discussed in Duflo (2012) even small registration procedures may work as strong deterrents. Because distance and transactional costs are directly related, the finding of a larger program effect in municipalities that were in close proximity to other municipalities offering Seguro Popular, is consistent with the idea that administrative barriers may have played a role in limiting program take-up.

In terms of the broader policy debate it must be emphasized not only that this cost should be balanced against the benefits that the program creates in other dimensions of welfare, but additionally that this distortion in the labor market requires qualifiers in two areas. First, it must be established whether the reduction in the ratio of formal to total employment is being driven by workers switching between jobs, or alternatively because of differential entry and retention. Second, since the creation or loss of certain types of jobs is likely to have different effects on outcomes such as tax collection, it is important to characterize the type of jobs that are being lost, transformed or created as a result of the program.

All in all, while the mechanisms through which Seguro Popular affected the labor market are not yet fully uncovered, a strong case for the external validity of these findings can be made on the grounds that this type of intervention is likely to have a smaller effect in countries where the degree of labor market segmentation is higher than that of Mexico. Thus, while it may still be inefficient to have a two tier social security system for a number of reasons, policy makers on the road to universal coverage should

not be deterred from pursuing a temporary expansion of non-contributory health services because of its potentially negative effects on formal employment.

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